with an experimental stage of the subject, which may, however, have considerable possibilities of actual utility in warfare.

In any future edition of his book the author would be well advised to cut out all unessential matter. He is not a safe guide on points of history or priority in relation to radio-telegraphic invention. His statement on p. 32 as to "later improvements" is absurdly inaccurate. On pp. 105 and 106 he misspells the name of Prof. E. Branly, the inventor of the metallic-filings coherer. On p. 173 he gives exclusive credit for the vacuum valve detector to Lee de Forest, apparently in entire ignorance that the Court of Appeals in the United States has confirmed Judge Mayer's decision that the de Forest audion is an infringement of the Fleming oscillation valve. He is also seemingly unaware that the so-called Dolbear system of wireless telegraphy was never operative. In spite of the fact that there is a free use of photographs of apparatus which are insufficiently described in the text, the reader who is desirous of learning what can be done in the radio-control of torpedoes will find a good deal of suggestive research described in this little book. J. A. F.

THE ACTION OF ENZYMES.

The Method of Enzyme Action. By Dr. J. Beatty. With Introduction by Prof. E. H. Starling. Pp. ix+143. (London: J. and A. Churchill, 1917.) Price 5*s*.

TWO-THIRDS of this book is devoted to an excellently clear and concise account of the facts and theories to be found in the books named in the preface, so far as they are connected with the action of enzymes. What is new, and the chief object of the book, is the suggestion of a hypothesis of enzyme action. The details can be adequately grasped only from the full description. It is based on two assumptions: (1) the possibility of "combination" between molecules, and (2) the loosening of internal bonds in one or both of the combining molecules as a result of this union. It is held that the action of all enzymes can be reduced to the combination with H and OH radicles derived from water. These radicles are "activated" by the power possessed by an enzyme of "attracting" one or the other. This is a general or unspecific property, but each enzyme has also a specific power of adsorbing some particular substrate. The author will, no doubt, admit that considerable further explanation is required as to the means by which the activation is effected, and criticism is difficult until more is known of the nature of the atom itself and of the way in which it is united to other atoms.

The hypothesis deserves to be kept in mind as more knowledge is gained of the action and the nature of enzymes. At present it is not easy to imagine ways of putting it to experimental test. Indeed, it must not be forgotten that the fundamental assumptions are not universally accepted. The present writer is inclined to think that the use of the word "combination," although very common, in speaking of the union between molecules and even of adsorption, is apt to lead to an obscuring of the great, salient facts of true chemical union. There are, as it seems, various stages of "combination," leading from adsorption, through molecular compounds, to cases where the change of properties is of the most striking kind.

It is somewhat unfortunate that the new hypothesis appears to involve the explanation of all catalytic action by the formation of intermediate compounds. This has been shown actually to take place in one case alone of heterogeneous catalysis, and since the compounds are only supposed to exist momentarily, it seems somewhat hopeless to expect a proof or disproof of their existence. The possibility of representing a reaction by a chemical equation does not necessarily show that it takes place in that way. The division of catalysts into inorganic and enzymes would be better replaced by that into homogeneous and heterogeneous, the latter to include enzymes. There are more differences between catalysis in homogeneous and heterogeneous systems than between inorganic heterogeneous catalysts and enzymes. In fact, more knowledge of the mechanism of heterogeneous catalysis is greatly to be desired. In future developments of his hypothesis the author might also find it of advantage to consider it more fully in the light of the doctrines of energetics.

W. M. B.

OUR BOOKSHELF.

Clinical Bacteriology and Haematology for Practitioners. By Dr. W. D'Este Emery. Fifth edition. Pp. xiii+plates xi+pp. 310. (London: H. K. Lewis and Co., Ltd., 1917.) Price 9*s*. net.

A NEW edition of Emery's "Clinical Bacteriology and Hæmatology" is always welcome, for it is a most useful book for the practitioner and for laboratory work. The general plan has been maintained in this fifth edition, but the text has been revised, some new matter added, and some more illustrations have been inserted.

In the section dealing with syphilis the McIntosh and Fildes method of performing the Wassermann reaction has been inserted in addition to the author's own method, and we think the author has been well advised to do this.

The Dreyer method of performing the agglutination test for typhoid fever is also described in full as well as earlier methods.

Cerebrospinal fever and the recognition of the meningococcus are dealt with more fully than previously, and the examination of carriers is described. Even now this section is none too long, and might be extended with advantage. We doubt if it is wise ever to rely on ordinary agar as a culture medium for this organism, as is suggested.

Malaria is described very briefly, and no mention is made that the crescents of sub-tertian fever are free in the blood-plasma and are not

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intra-corpuscular. We are surprised also to find no reference to amœbic dysentery; with many cases now coming from abroad the practitioner is quite likely to meet with this disease. While pointing out these few slight blemishes, we can cordially recommend this book as on the whole a simple and reliable guide to clinical bacteriology and pathology.

Soil Conditions and Plant Growth. By Dr. E. J. Russell. Third edition. Pp. viii + 243. (London: Longmans, Green, and Co., 1917.) Price 6*s*. 6*d*. net.

THIS book is, as the title implies, concerned with the relationship between soil and plant. After an introductory historical account of the subject the author describes the constitution of the soil and the various factors of plant growth. In the development of these topics and of the question of the relation of the plant to its soil environment, the reader is kept constantly in touch with the best original work at home and abroad.

The author has made numerous additions in his third edition, and has considerably expanded those portions treating of the biological conditions in soils. He has also added a chapter on the colloidal properties of the soil, in which he brings the reader abreast of the recent researches and disputes of Continental workers, as well as the latest Rothamsted work on the interaction of dilute acids and soil colloids.

It is superfluous to say that the book is well written. There is an ample bibliography, which should be invaluable to the investigator in any branch of the subject.

The study of the relationship between soil and plant is exceedingly complex. Progress can only be made by studying the soil in every aspect of importance to the plant. It is a pity that so much labour should have been expended during past years in haphazard manurial trials, designed to instruct the farmer, but yielding generally a scanty harvest of accurate information. Soil investigators owe a debt to Dr. Russell and his predecessors at Rothamsted for enlarging the study of the soil into a respectable field of scientific activity. G. W. ROBINSON.

Lezioni di Antropologia. By Prof. Fabio Frassetto. Vol. iii. Pp. xiii + 422. (Bologna: Mareggiani, 1917.) Price 20 lire.

IN the thirteen lectures contained in this volume Prof. Frassetto covers that part of his course which is devoted to the limbs—their evolution, development, and morphology. In his lectures dealing with the methods which are to be applied to the measurement of bones and to the exact estimate of their anthropological characters he has introduced much that is new and valuable. All through these lectures is reflected that spirit of mutual understanding which has existed between the anatomists of Italy and England since Harvey's time. There is no better summary of the contributions which British anatomists have made to physical anthropology than is to be found in these clearly written and excellent lectures by Prof. Frassetto. A. K.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Radiation of the Stars.

I AM sorry that the only reply I can make to Prof. Eddington's remarks in NATURE of July 5 is that his amended equation seems to me neither to be true nor to lead to his supposed laws. But perhaps I may be permitted to offer a few remarks on the general problem.

Suggested sources of stellar energy fall into two broad classes—gravitational and electrical, the latter including chemical and radio-active sources. Of the former the Helmholtz contraction is by far the most powerful source of energy; the contraction of our sun from a state of infinite rarity would provide energy for about 20,000,000 years of radiation at the present rate.

An upper limit to the capacity of electrical sources can be calculated in a way I have not seen elsewhere. Our sun's mass is 2×10^{33} grm., its radiation about 4×10^{33} ergs per second. Thus to radiate for 20,000,000 years (or, say, 7×10^{14} sec.) at its present rate, each gram of matter must provide on the average 14×10^{14} ergs of energy. A gram of matter contains 3×10^{23} negative electrons, so that the average electron must provide $4{\cdot}7 \times 10^{-9}$ ergs of energy, corresponding to a fall through a potential difference of 10 electrostatic units, or 3000 volts. This is the energy of falling from infinity to a distance of only $4{\cdot}7 \times 10^{-10}$ cm. from a nucleus $10e$, and so is probably much greater than any energy actually available from changes of electrical structure; it is, of course, enormously greater than any known ionisation potentials.

It accordingly looks as though the Helmholtz contraction will provide much more energy than any other source, and we must apparently adjust our views to the time-scale set by the contraction theory.

If this is accepted, it will be obvious that the calculation of stellar temperatures and emissions of energy cannot be a steady-state problem at all. I do not see how, in any case whatsoever, any new knowledge can be gained from calculations which assume the star to be in a steady state, for the calculated rate of emission can come to nothing but the previously assumed rate of generation of energy inside the star. For progress to be made, this big and difficult problem must, I think, be attacked on dynamical, and not on statical, lines.

July 7.

I HOPE I may state my grounds for disbelief in Prof. Eddington's results more clearly. The results are readily combined in the one result that the total emission depends only on M, and varies as M. Prof. Eddington claims to obtain this result in two ways. In his original paper he assumes (*M.N.*, vol. lxxvii., p. 20) a rate of generation $4\pi M\epsilon$, and after much calculation obtains a result which, on introducing the omitted constants, reduces (*l.c.* p. 29, equations (29) and (25)) to exactly $4\pi M\epsilon$, proving my point. Again, in NATURE of June 14, Prof. Eddington claims to obtain a result which I do not understand, but which is necessarily contradictory to the foregoing, since the emission cannot now involve ϵ at all. He apparently says:—"Tell me the mass of a star and I will tell you its output of radiation without knowing the rate at which energy is being generated in its interior; I

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